

parallel motion; whereas, it is entangled and broken up in the portion where the sinuous motion of the water occurs.'

"It will be observed that this explanation is to a certain extent the same as that subsequently given by Prof. Osborne Reynolds in the letter to NATURE, from which I have quoted, and to which I refer under heading (2).

"The experiments of Prof. Reynolds, which he cites himself, are entirely different from those of which I have given an account, and seem to me to have but very little bearing upon the behaviour of water in the conditions under which my experiments were conducted.

"With regard to the differences of state in the water in the light band, I will not trespass on your time with a repetition of the arguments which I have already published, and which have not hitherto been answered, but I would point out that the experiments you have seen to-night form a most striking method of putting the theory to the test by actually placing liquid under the condition of the thin border, and in obtaining when this is done, results which are absolutely different from those which were given by the thick film or sheet of water. This is one of the strongest possible arguments in favour of the views which I have advanced, inasmuch as the results of the experiment seem to have surprised some of the greatest authorities on the subject.

"Coming to (3), I would first remark, though it has never been distinctly claimed, that the water charged with air actually represented real stream-line motion, in my first paper it was stated that 'there was no difference whether the smallest quantity of air was present, or so large a quantity as to render the slide opaque, as the general behaviour of the flow of water was practically unaffected by the presence of the air.'

"Though this remark only applied to the general nature of the results obtained, the experiments brought out very strongly, I believe for the first time, various points which had long been known to the naval architect, but which had not been actually made visible to the eye. Prof. Osborne Reynolds, however, states that 'air bubbles are the most misleading bodies that can possibly be chosen to indicate the lines of motion in a fluid in sinuous motion.' This remark led me to consider the very appropriate experiment of trying the effect of first taking the air-charged water upwards and then taking it downwards under the same conditions. Inasmuch as the air in one case was trying to rise upwards through the water, which is moving in the same direction, whereas in the other case it is trying to move in the opposite direction, it is perfectly certain that if there was much difference in the flow owing to the presence of air, it would become marked under those circumstances.

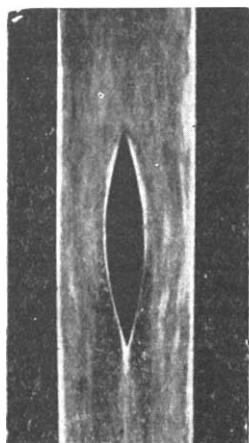


FIG. 1.

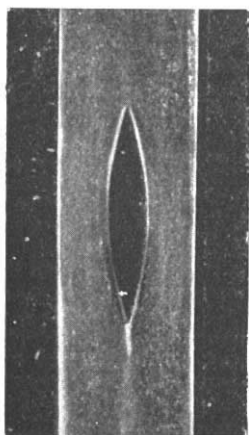


FIG. 2.

"The two photographs, Figs. 1 and 2, represent the results of this experiment, and I venture to think that no one would be able to tell from the photographs themselves where the air-charged water was flowing upwards or where it was flowing downwards. This, I think, shows in a very striking way the comparatively small effect which the presence of air under suitable conditions, has upon the flow of the water, and it is quite contrary to that which Prof. Osborne Reynolds seems to anticipate."

The truth of the whole matter appears to be this, that as far as Prof. Osborne Reynolds has dealt with the behaviour of air in water, it has been under conditions represented by plates suddenly immersed or moving through still water, in which case air has been in a state of bubbles of large size, whereas in my own experiments, the air has been broken up into very minute bubbles. The behaviour in the two cases has been as different as that of a mass of water, say from a bucket, thrown through the air, and the finely-divided particles of moisture in a fog or mist. The effect of viscosity makes all the difference in the two cases. As, however, I have never had an opportunity of seeing Prof. Osborne Reynolds's experiments, and as he has never witnessed mine, perhaps his disagreement with me is, after all, only another illustration of the old fable of the chameleon.

H. S. HELE-SHAW.

University College, Liverpool, December 21, 1898.

### Etherion, a New Gas?

SOME months ago the discovery of a new gas, by Mr. C. Brush, was announced by nearly all the scientific periodicals of the world, which was said to be endowed with quite extraordinary properties; as, for instance, density one ten-thousandth of hydrogen, molecular velocity and heat conductivity hundred times that of hydrogen!

It seemed strange to me that such tremendous assumptions should be based on no more convincing arguments than the experiments (Mr. Brush's) reported therein, on the relative increase of condition of heat in rarefied air, when glass powder, contained in the same vessel, was being heated, and I was waiting eagerly for the publication of the original account (Mr. Brush's), since it struck me that all observed phenomena could be explained by the well-known properties of water vapour.

I was glad to learn afterwards, from a paper in the *Chemical News* (November 4, p. 221), that this is also the opinion of Sir William Crookes, undoubtedly the greatest authority in this kind of research.

Now the original paper (Mr. Brush's) appeared in *Science* for October 14. It has been already the subject of a severe criticism by "A Physicist" in the *Chemical News* (December 2, p. 277), as it does not contain indeed any further argument for Mr. Brush's hypothesis. I do not think it superfluous, however, to warn in NATURE, too, against an excessive credulity in this matter, and to point at some facts not yet emphasised sufficiently by other sceptics.

First I must mention, for the sake of those readers who are not sufficiently acquainted with these things, that the conduction of heat by gases, when not disturbed by convection currents, is independent of the pressure, until this comes down to several millimetres of mercury; then it begins to decrease, at first very slowly, then faster, until it becomes nearly proportional to the pressure, at the highest rarefactions; and the differences of conduction in various gases, very marked at higher pressures, are much less at pressures of several millionths of an atmosphere.

These facts, which are in strictest accordance with the kinetic theory of gases, as I have shown in the *Phil. Mag.* (August 1898), have been investigated besides by other observers very carefully, also by Mr. Brush himself, and form the object of a very interesting paper of his in the *Phil. Mag.* (January).

Mr. Brush found that the conduction of heat is increased very much at high exhaustions in respect to other gases at corresponding pressures, when glass powder is heated (but remaining always much smaller than at normal pressures), and he infers from this that a gas of enormous conductivity is given off by heated glass powder.

Now to me the main point seems to be—how did Mr. Brush measure these low pressures? By an improved form of the Macleod gauge, which seems to be very suitable for dry gas, but of course, like every Macleod gauge, is quite unfit for gases where moisture or other condensable vapours are present, since then it indicates only the partial pressure of the not condensable gas. He did not use any drying agents, since they absorbed the gas in question, therefore the indications of the gauge are of no value whatever as to the total gas pressure.

When Mr. Brush measured a gas pressure of 0.38 millionth, and found the conduction to be about forty times that of dry air at this pressure, he may have had in reality a pressure of 0.38 millionth of air, and besides a pressure of 20 millionth of

water vapour, which of course would be sufficient to account for that large effect.

Sir William Crookes mentions that, according to his experiments, water vapour produces a greater conduction at very low pressures than air; but I decidedly disbelieve the difference to be in any way similar to the numbers given by Mr. Brush, in consequence of a reasoning mentioned at the end of my *Phil. Mag.* paper, and I think the simplest explanation to be afforded by this source of errors vitiating the pressure indications.

It is no mere hypothesis but a certain fact that water vapour is being evolved by heated glass, and probably many other substances. Sir William Crookes gathers quite a number of arguments for it from his own researches, amongst others spectroscopical proofs. The same opinion was put forward by Kundt and Warburg, who were led to it by the very same sort of experiments on conduction of heat as Mr. Brush's, which they made as early as 1875 (*Poggendorff's Annalen*, 156, p. 177). Further investigations on the hygroscopical properties of glass, and on means for partially removing them, were published by Warburg and Ihmori (*Wiedemann's Annalen*, 27, p. 481).

I cannot prove, of course, that there is no new gas evolved, but I maintain that whatever facts Mr. Brush has put forward as an evidence for its existence, can be explained quite simply by the presence of water vapour (perhaps also other condensable vapours), which he seems to have overlooked. I do not think it necessary to go into details, and to analyse more thoroughly the—rather fantastic—speculative part of the paper, where scarcely any statement is not open to serious objections.

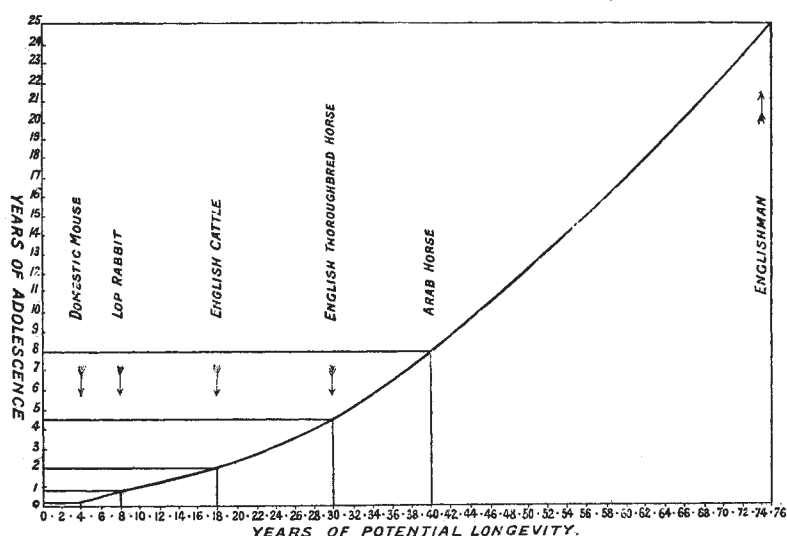
Although thus I differ from Mr. Brush very much in respect to the interpretation of his recent results, I think his elaborate experimental investigations, as reported in his *Phil. Mag.* paper, to be of great value for the theory of these phenomena; certainly it would be very desirable that he might carry on those researches, as he promised there to do.

M. SMOLUCHOWSKI DE SMOLAN.

Vienna, University.

### The Curve of Life.

THE relationship between the duration of adolescence and potential longevity in different species of mammals has repeatedly been the subject of speculation. M. P. Flourens, in his work



on "Human Longevity," made the ratio between the two periods as 1 to 5; Buffon had previously concluded that it was as 1 to 7. In neither case were the data sufficiently numerous and trustworthy to make these figures generally accepted. In the course of some investigations on the variability of the adolescent period in different breeds of the same species among certain well-known mammals, I have satisfied myself that a relationship exists between the duration of growth and the length of an animal's natural life; although it is evidently not of the kind suspected by the older writers. It may be stated as follows:—The ratio of length of adolescence to length of life in the

shortest lived mammals is proportionally less than it is in longer lived mammals. For example, the period of growth and development of the domestic mouse is, according to my informant, a breeder of these small rodents, about three months. Its natural lifetime is four years. In other words, the mouse may be expected to live about fifteen times its adolescent period as a mature animal. The Arab horse, according to a well-known authority, arrives at maturity in about eight years, its lifetime is about forty years; that is to say, the animal lives four times the length of its adolescence as an adult. Man, on the other hand, who only completes his growth by the union of the sternal epiphysis of the clavicle to its shaft at the age of twenty-five, has, after passing his fiftieth year, or "the middle arch of life," to use Dr. Farr's phrase, only another twenty-five years' expectation of life. His potential longevity accordingly foreshadows a period of maturity not greater than twice the length of his youth.

I have obtained, through the kindness of numerous correspondents interested in breeding and rearing of farm and other domestic animals, the approximate lengths of these two periods in a few well-known mammals; and the accompanying diagram shows the relations between growth and longevity among the same animals plotted as a definite curve. This result was entirely unexpected by me, and it may be interesting to some of your readers.

W. AINSLIE HOLLIS.

Hove.

### The Alleged Destruction of Swallows and Martins in Italy.

IN your issue of December 22, 1898, I read the report of a conference held by the Society for the Protection of Birds, at which a paper was read on the decrease of swallows and martins coming to England, giving, as a reason for this decrease, the netting of thousands of these birds on their arrival at the Italian shores, and their subsequent consumption as food.

May I venture to remark that, during a residence of some years in Italy, I have never once seen a swallow, or any member of its family, exposed for sale, and that I have never known, or heard of, an instance of their being netted in the manner described, though I am well acquainted with nearly every part of the Peninsula.

Since reading the above mentioned accusation against Italy, I have asked several Italians whether they knew of such a practice, and am informed that it is simply non-existent, the swallow being, perhaps, the one bird in this country which is regarded with a kind of sentiment by all classes, as the harbinger of spring.

Swallows, moreover, do not arrive on these coasts in a state of exhaustion, and to net them would be no easy feat.

A few isolated cases of the cruel method of capturing them with artificial flies may occur, but not more so than in England.

The Italian may be ruthless in his destruction of other birds, but is certainly not a destroyer of the *Hirundinidae*.

Of the similar charge made against the French, I am not in a position to judge, but I imagine that the cause for the decrease of the *Hirundinidae* in England may lie in quite another direction, and may be attributable to some equivalent decrease of their favourite insects in our islands, or in some atmospheric and climatic change. Italy, I am convinced, is

not responsible in any way for it.

RICHARD BAGOT.

Roma, December 27, 1898.

### RADIATION PHENOMENA IN THE MAGNETIC FIELD.

IN the spring of 1897 the scientific world became indebted to Dr. Zeeman for the observation that when a source of light is placed in a strong magnetic field the spectral lines of the light emitted by that source suffer